

October 28, 1952

Dear Elie:

Your paper "Sur la determination genetique de la lysogenie" just came, and we have read it with considerable pleasure. It is a very lucid statement of the problem. There is very little that we can suggest for its improvement, but we should perhaps ask your consideration of the following:

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- 1) Reference 6 is now in page proof: Genetics, 1952, 37, 720.
- 2) There has been a certain amount of confusion on the mechanism owing to the statement by Hayes of the lambda theory of recombination. He should be credited with this hypothesis (ref. 9) in your paragraph 3, page 2.
- 3) The basic stocks should be thoroughly characterized— there has already been some confusion in other papers that may make it difficult to correlate the work in different laboratories. On p. 6, W-677 (and its ancestor, W-1 as should be stated) are characterized most extensively in reference 5. I am not clear as to the nature of "K-12 S⁺". Is it W-1485 (as described in our ms.)? If so, it should be V₁^S to begin with. If it was already V₁^F when you received it, it must be something else, and this should be ascertained. In any case, its history should be given in enough detail so that we can tell just what it is.
- 4) Several of us had some difficulty in a full understanding of the first paragraph, p. 14, to the extent that one suggested that "prendrait" should be "perdrait". Since there has been this difficulty, perhaps the hypothesis and reasoning for the experiment on p. 14 lines 5-8 should be explained more fully. I think it would be premature to discuss our transduction experiments, which seem to have very little direct bearing on the crossing results. One solution, perhaps, is to delete this paragraph, but this is entirely for your own judgement.
- 5) With full respect for your parents, may I suggest that many of your readers will confuse the author of the present paper with that of ref. 1 and 16. Would some notation (sr./jr.? or something else) be appropriate? Ref. 16 may also be confusing as it presents a conception of phage as an endogenous unit, even for its parasitic behavior, or have I perhaps an incomplete understanding of it. The main accomplishment of your present work would be the support it gives for prophage not as a cytoplasmic, but as a nuclear determinant.

6) Reference 7 spell Heredity Title: Microbienne.

Yours sincerely

for
Esther and Joshua Leder-
berg

*2 different stocks that we
have sent out with different
labels have been called "S".*